Transactions of Fusion Technology, Vol. 26, Number 4T, Part 2, December 1994: p. xiii. This paper was presented at *The Fourth International Conference on Cold Fusion,* 1993. (ICCF-4) Lahaina, Maui, Dec. 6-9, by Eugene Mallove in place of Julian Schwinger. Schwinger died in July 1994.

Cold Fusion Theory

A Brief History of Mine

As Polonius might have said: “Neither a true-believer nor a disbeliever be.” From the very beginning in a radio broadcast on the evening of March 23, 1989, I have asked myself—not whether Pons and Fleischmann are right—but whether a mechanism can be identified that will produce nuclear energy by manipulations at the atomic-the chemical-level. Of course, the acceptance of that interpretation of their data is needed as a working hypothesis, in order to have quantitative tests of proposed mechanisms.

As a long-time nuclear physicist, the knee jerk reaction to the idea of a D-D reaction without significant neutron production brought in words like ^4He and Mössbauer effect. I tried, without success, to contact P(ons) and F(leischmann), to the point of sending a letter to the Los Angeles Times, which was garbled in the editing process. Finally, with the help of a friend, contact was made in the early part of April and I went to Salt Lake City.

There, I was assured that they knew about ^4He, and was shown a peak in a spectroscopic read-out which, I was told, was ^4He. Soon after my return to Los Angeles, references to ^4He disappeared, to resurface only relatively recently.

I do not have to—but shall—remind you of the two fundamental problems that the acceptance, of P & F’s excess heat as nuclear in origin, entails.

1. What accounts for the absence of particles that are familiar in ordinary hot fusion, such as the neutrons of $D + D \rightarrow n + {}^3He$ and the high energy γ-ray of $D + D \rightarrow \gamma + {}^4He$? Very early in my thinking I added the conventional reaction $p + D \rightarrow \gamma + {}^3He$. Why? Mostly because it would also be there. One cannot produce heavy water without some contamination by light water.
2. Hot fusion relies on achieving enough kinetic energy to overcome the Coulomb repulsion between like charges. How then can cold fusion, operating far below those levels, ever achieve fusion? Incidentally, I have read, and heard, that my solution to the Coulomb barrier problem is to forget it! Not even an absent-minded professor (which I am not) would go that far. Critics should learn to operate within the bounds of sanity.

My first attempt at publication, for the record, was a total disaster. “Cold Fusion: A Hypothesis” was written to suggest several critical experiments, which is the function of hypothesis. The masked reviewers, to a person, ignored that, and complained that I had not proved the underlying assumptions. Has the knowledge that physics is an experimental science been totally lost?

The paper was submitted, in August 1989, to Physical Review Letters. I anticipated that PRL would have some difficulty with what had become a very controversial subject, but I felt an obligation to give them the first chance. What I had not expected—as I wrote in my subsequent letter of resignation from the American Physical Society—was contempt.

"Hypothesis" was eventually published, after protracted delays, in a 1990 issue of a German periodical. Does it have any significance in 1993? I cite the following excerpts:

". . . this cold fusion process (of P & F) is not powered by a DD reaction. Rather it is an HD reaction, which feeds on the small contamination of D_2O by H_2O.",

and:

"The HD reaction $p + D \rightarrow {}^3He$ does not have an accompanying γ-ray; the excess energy is taken up by the metallic lattice of Pd alloyed with D."

and finally:

"... concerning the oft repeated demand for a control experiment using H_2O, one should note the possibility of a converse effect of the HD reaction: Through the natural presence of D_2O in ordinary water, such control experiments might produce an otherwise puzzling amount of heat."

A following paper, entitled: "Nuclear energy in an atomic lattice, 1," was sent directly to another German periodical, in November of 1989. As of today, the only memorable part is a quotation from Joseph Priestly:

"In this business, more is owed to what we call chance-that is, to the observation of events arising from unknown causes-than to any preconceived theory."

The editor thought it necessary to add a total disclaimer of responsibility, ending with: "We leave the final judgment to our readers." In my naivety I had thought that was always so. When part 2 of NEAL was submitted, it was simply rejected. The fix was in.

I gave a talk with the same title—"Nuclear Energy in an Atomic Lattice"—at Salt Lake City in March of 1990. The HD hypothesis-of the dominance of the pd reaction-has the pragmatic advantage of suppressing neutron production at the level of excess heat generation.

But, to quote from that lecture: "... a well trained hot fusioneer will instantly object that there must also be a 5.5 MeV γ-ray. He will not fail to point out that no such radiation has been observed. Indeed."

"But consider the circumstances of cold fusion. (The quotation continues.) At very low energies of relative motion, the proton and deuteron of the HD reaction are in an s-state, one of zero orbital angular momentum, and therefore of positive orbital parity. The intrinsic parities of proton, deuteron, and ^{3}He are also positive. Then, the usually dominant electric dipole radiation-which requires a parity change-is forbidden."

I turn from 'missing' radiation to Coulomb repulsion, and quote:

"... treatments of nuclear fusion between positively charged particles (usually) represent the reaction rate as the product of two factors. The first factor is a barrier penetration probability. It refers entirely to the electric forces of repulsion. The second factor is an intrinsic nuclear reaction rate. It refers entirely to nuclear forces. This representation ... may be true enough under the circumstances of hot fusion. But, in very low energy cold fusion one deals essentially with a single state, or wave function, all parts of which are coherent. It is not possible to totally isolate the effect of the electric forces from that of the nuclear forces: The correct treatment of cold fusion will be free of the collision dominated mentality of the hot fusioneers."

To speak of transferring energy to the lattice is to invoke lattice excitations, or phonons. At about the time of the Salt Lake City meeting, or shortly after, I became dissatisfied with my treatment, and began to reconstruct phonon theory. A note entitled "Phonon representations" was submitted to the Proceedings of the National Academy of Sciences in June of 1990. The abstract reads:

"The gap between the nonlocalized lattice phonon description and the localized Einstein oscillator treatment is filled by transforming the phonon Hamiltonian back to particle variables. The particle-coordinate, normalized wave function for the phonon vacuum state is exhibited."

A month later, I submitted a second note with the title "Phonon dynamics." The abstract reads:

"An atomic lattice in its ground state is excited by the rapid displacement and release of an atomic constituent. The time dependence of the energy transfer to other constituents is studied ... ."

The third and last note is called "Phonon Green's function." Its abstract is:

"The concepts of source and quantum action principle are used to produce the phonon Green's function appropriate for an initial phonon vacuum state. An application to the Mössbauer effect is presented."

I remind you that the Mössbauer effect refers to "an excited nucleus of an atom, imbedded in a lattice, (that) decays with the emission of a γ-ray," thereby transferring momentum to the lattice." There is a certain probability ... that the phonon spectrum of the lattice will remain unexcited, as evidenced by the absence, in the γ-ray energy, of the red-shift associated with recoil energy."

A casual explanation of the Mössbauer effect has it that the recoil momentum is transferred to the lattice as a whole so that the recoil energy, varying inversely with the mass of the entire lattice, is extravagantly small. As Pauli would say, even to God, "Das ist falsch!"

The spontaneous decay of a single excited atom in the lattice is a localized event, the consequences of which flow at finite speed, out into three dimensional space, weakening as they travel. This is a microscopic event, with no dependence on macroscopic parameters such as the total mass of the lattice.

Unmentioned in the abstract, but of far greater importance, is another situation. To quote: "What happens if the momentum impulse ... is applied, not to one, but all lattice sites?" The reader is invited to "recall that the lattice geometry is not absolute, but relative to the position of the center of mass for the entire system. Thus (the injected energy) can be read as the kinetic energy transferred to the lattice as a whole." More of this shortly.

In the last month of 1990, I went to Tokyo. The occasion was the 100th anniversary of the birth of a famous Japanese physicist, perhaps most familiar for his part in the Klein-Nishima formula for Compton scattering. On a day that, to my surprise, I found uncomfortably close to another-infamous-day, I delivered a lecture on: "Cold Fusion—Does It Have a Future?" The abstract reads:

"The case against the reality of cold fusion is outlined. It is based on preconceptions inherited from experience with hot fusion. That cold fusion refers to a different regime is emphasized. The new regime is characterized by intermittency in the production of excess heat, tritium, and neutrons. A scenario is sketched, based on the hypothesis that small segments of the lattice can absorb released nuclear energy."

I pick up the last sentence of the abstract with this quotation from the text:

"If the γ-rays demanded by the hot fusioneers are greatly suppressed, what agency does carry off the excess energy in the various reactions? One must look for something that is characteristic of cold fusion, something that does not exist in the plasma regime of hot fusion. The obvious answer is: the lattice in which the deuterium is confined.

Imagine then, that a small, but macroscopic piece of the lattice absorbs the excess energy of the HD or DD reaction. ... I advance the idea of the lattice playing a vital role as a hypothesis. ... Intermittency is the hallmark of cold fusion. ... Does the lattice hypothesis have a natural explanation for intermittency? ... a close approach to saturation loading is required for effective fusion to take place. But, surely, the loading of deuterium into the palladium lattice does not occur with perfect spatial uniformity. There are fluctuations. It may happen that a microscopically large-if macroscopically small-region attains a state of such lattice uniformity that it can function collectively in absorbing the excess nuclear energy that is released in an act of fusion. And that energy can initiate a chain reaction as the vibrations of the excited ions bring them into closer proximity. So begins a burst. In the course of time, the increasing number of vacancies in the lattice will bring about a shut-down of the burst. The start-up of the next burst is an independent affair. (This picture is not inconsistent with the observation of extensive cracking after long runs.)

What answer did I give, just three years ago to “Does cold fusion have a future”? I said: “I have little hope for it in Europe and the United States—the West. It is to the East, and, specifically, to Japan that I turn.”

Inspired by good soba and sushi, I dashed off a short addendum that Progress of Theoretical Physics received in January and published in April of 1991. The abstract of “Nuclear Energy in an Atomic Lattice-Causal Order” is:

“The extremely small penetrability of the Coulomb barrier is generally adduced to dismiss the possibility of low energy (cold) fusion. The existence of other mechanisms that could invalidate this logic is pointed out.”

Here are excerpts. “... Implicit in this line of thought (of negligible penetrability) is the apparently self-evident causality assignment that has the release into the surrounding environment, of energy at the nuclear level, occur after the penetration of the Coulomb barrier. One would hardly question that time sequence when the environment is the vacuum. But does it necessarily apply to the surrounding ionic lattice? ... another reading is possible, one in which the causal order is reversed. Why? Because, in contrast with the vacuum, the lattice is a dynamical system, capable of storing and exchanging energy.

The initial stage of the new mechanism can be described as an energy fluctuation, within the uniform lattice segment, that takes energy at the nuclear level from a Pd or dd pair and transfers it to the rest of the lattice, leaving the pair in a virtual state of negative energy. ...

For the final stage ... consider the pd example where there is a stable bound state: ^{3}He. If the energy of the virtual state nearly coincides with that of ^{3}He, a resonant situation exists, leading to amplification, rather than Coulomb barrier suppression.

It would seem that two mechanisms are available ... . But are they not extreme examples of mechanisms that in general possess no particular causal order?”

The last lecture on cold fusion was delivered-twice-in the Fall of 1991, to celebrate the birthdays of former students, one of whom is at MIT, a hotbed of hot fusioneers. The cover title: “A Progress Report,” injects a bit of my own nostalgia. Not long after the simultaneous arrival of myself at Berkeley and World War II, Robert Oppenheimer gave a lecture with that title. As he explained, it meant only that time had elapsed. That also applied to the first part of my birthday lectures—“Energy Transfer in Cold Fusion”—with one exception:

“I note here the interesting possibility that the ^{3}He produced in the Pd fusion reaction may undergo a secondary reaction with another deuteron of the lattice, yielding ^{5}Li (an excited state of ^{6}Li lies close by). The latter is unstable against disintegration into a proton and ^{4}He. Thus, protons are not consumed in the overall reaction, which generates

To this I add, as of some time in 1992, that observations of ^{4}He, with insufficient numbers to account for total heat generated, are consistent with the preceding suggestion. The initial Pd reaction produces heat, but no ^{4}He. The secondary reaction generates heat and ^{4}He. There may be more total heat than can be accounted for by ^{4}He production. The smaller the ratio of secondary to primary rates, the more the ^{4}He production will be incapable of accounting for the heat generation.

The second part of "A Progress Report" is entitled: "Energy Transport in Sonoluminescence." What is that?

The text begins with:

"The suggestion that nuclear energy could be transferred to an atomic lattice is usually dismissed . . . because of the great disparity between atomic and nuclear energy scales; of the order 10^7, say. It is, therefore, of great psychological importance that one can point to a phenomenon in which the transfer of energy between different scales involves (an) amplification of about eleven orders of magnitude."

"It all began with the sea trials, in 1894, of the destroyer HMS Daring. The onset, at high speeds, of severe propeller vibrations led to the suggestion that bubbles were forming and collapsing-the phenomenon of cavitation. Some 23 years later, during World War I, Lord Rayleigh, no less, was brought in to study the problem. He agreed that cavitation, with its accompanying production of pressure, turbulence, and heat, was the culprit. And, of course, he devised a theory of cavitation. But, there, he seems to have fallen into the same error as did Isaac Newton, who, in his theory of sound assumed isothermal conditions. As Laplace pointed out in 1816, under circumstances of rapid change, adiabatic conditions are more appropriate.

During World War I, the growing need to detect enemy submarines led to the development of what was then called (by the British, anyway) subaqueous sound-ranging. The consequent improvement in strong acoustic sources found no scientific applications until 1927. It was then discovered that, when a high intensity sound field produced cavitation in water, hydrogen peroxide was formed. Some five years later came a conjecture that, if cavitation could produce such large chemical energies, it might also generate visible light. This was confirmed in 1934, thereby initiating the subject of sonoluminescence; SL. I should, however, qualify the initial discovery as that of incoherent SL, for, as cavitation noise attests, bubbles are randomly and uncontrollably created and destroyed.

The first hint of coherent SL occurred in 1970 when SL was observed without accompanying cavitation noice. This indicates that circumstances exist in which bubbles are stable. But not until 1990 was it demonstrated that an SL stream of light could be produced by a single stable cavity.

Ordinarily, a cavity in liquid is unstable. But it can be stabilized by the alternating cycles of compression and expansion that an acoustic field produces, provided the sonic amplitudes and frequencies are properly chosen. The study of coherent SL, now under way at UCLA under the direction of Professor Seth Putterman, has yielded some remarkable results.

What, to the naked eye, appears as a steady, dim blue light, a photomultiplier reveals to be a clock-like sequence of pulses in step with the sonic period, which is of the order of 10^{-4} seconds. Each pulse contains about 10^5 photons, which are emitted in less than 50 pico seconds, that is, in about 10^{-11} seconds.

When I first heard about coherent SL (my term), some months ago (June 1991), my immediate reaction was: This is the dynamical Casimir effect. The static Casimir effect, as usually presented, is a short range non-classical attractive force between parallel conducting plates situated in a vacuum. Related effects appear for other geometries, and for dielectric bodies instead of conductors.

A bubble in water is a hole in a dielectric medium. Under the influence of an oscillating acoustical field, the bubble expands and contracts, with an intrinsic time scale that may be considerably shorter than that of the acoustical field. The accelerated motions of the dielectrical mateial creates a time dependent-dynamical-electromagnetic field, which is a source of radiation. Owing to the large fractional change in bubble dimensions that may occur, the relation between field and source could be highly nonlinear, resulting in substantial frequency amplification.

The mechanisms that have been suggested for cold fusion and sonoluminescence are quite different. (So I wrote in 1991.) But they both depend significantly on nonlinear effects. Put in that light, the failures of naive intuition are understandable. So ends my Progress Report."

In the more than two years that have elapsed since the birthday lectures, I have concentrated on the theory of coherent sonoluminescence. Why? Because, of the two physical processes that naive intuition rejects, it is coherent SL that exists beyond doubt. (No, Mr. Taubes, not even you could cry fraud. Too many people have seen the light.) With the advantage of reproducible data, under variable circumstances, constructing a convincing theory for coherent SL should be, by far, the simpler. That, in turn, should supply analogies for theory construction in a domain that is characterized experimentally by "irreproducibility and uncontrollable emission in bursts."

My gut feeling about the Casimir effect, in a dynamical role, first needed some brushing up in the static domain, which I had not thought about for 15 years. My progress in doing that, along with needed simplifications, is recorded in four notes, published in 1992. Two of them share the title "Casimir energy for dielectrics." Each note acknowledges the stimulation provided by the phenomenon of coherent SL. I give only this brief excerpt concerning the action quantity W_0:

"What the static and dynamic Casimir effects share is the reference to the quantum probability amplitude for the preservation of the photon vacuum state: (exponential of *iWo).* That the vacuum persistence probability is less than one, in a dynamical situation where photons can be emitted, is expressed by a nonzero imaginary part of W_0: ... . In a static situation where W_0 is real, the shift in phase associated with a time lapse, ... identifies E, the energy of the system, ..."

In the latter part of 1992, and in 1993, five papers were submitted under the cover title "Casimir light." The individual ones are called, successively: A glimpse; The source; Photon pairs; Pieces of the action; and, Field pressure. The first three notes adopted the over-simplification that the bubble collapse-the source of radiant energy-is instantaneous. "Pieces of the action" begins "to remove the more egregious aspects of that treatment." The Abstract reads:

"More realistic dynamics for the collapsing dielectric fluid are introduced in stages by adding contributions to the Lagrangian that forms the action. The elements are kinetic energy, Casimir potential energy, air pressure potential energy, and electromagnetic coupling to the moving dielectric. There are successful tests of partial collapse time and of minimum radius." This paper ends with a veiled question:

"If, as it would seem, a mechanism exists that transfers kinetic energy of a macroscopic body into energy of microscopic entities, could there not be-in a different circumstances-a mechanism that transfers energy of microscopic entities into kinetic energy of a macroscopic body?" What, in 1991, seemed to be only a pairing of two intuitively improbable phenomena ("The mechanisms that have been suggested for cold fusion and sonoluminescence are quite different."), now emerges as related ways of transferring energy between macroscopic and microscopic objects.

"Casimir light: Field pressure" begins with a question:

"How does a macroscopic, classical, hydromechanical system, driven by a macroscopic acoustical force, generate an astonishingly short time scale and an accompanying high electromagnetic frequency, one that is at the atomic level?"

In response, "I offer the hypothesis that light plays a fundamental role in the mechanism. Provocatively put:

The collapse of the cavity is slowed abruptly by the pressure of the light that is created by the abrupt slowing of the collapse."

The hypothesis becomes more quantitative with this supplement:

"The conditions for light emission are at hand when the fluid kinetic energy becomes independent of t (time) for a short time interval, and that similar remarks apply immediately after the emission act. In effect, one is picking out the circumstances for spontaneous radiation, from a coherent state of definite energy, to another such state of definite, lower energy."

The equation of motion-along with the conservation laws-that is supplied by the action principle, leads to a picture of what happens during abrupt slowing." Just before that begins, there is no significant field, . . . Then the field strength rises rapidly in the vacuum region, giving a positive value to the (outward pressure). . . . the slowing has begun. That process will cease when the field, flowing at the speed of *light* toward the outer dielectric region, has produced the countering pressure."

"The somewhat mysterious initial hypothesis has emerged clarified, as an unusual example of a familiar fact-spontaneous emission of radiation by an electric system is a single indivisible act that obeys the laws of energy and momentum conservation."

Now, finally, returning to the 1991 "Causal order" note, for the example of the reaction

$$p + d \rightarrow {}^{3}He + \text{lattice energy},$$

one also recognizes this as a single, indivisible act.

So ends this Progress Report.

JULIAN SCHWINGER